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XVI.—*On the recent Foraminifera.*

By William Clark, Esq.

To the Editors of the Annals of Natural History.

Gentlemen, Norfolk Crescent, Bath, Dec. 1, 1849.

There appeared in the 'Annals' for May 1849, a paper of mine on the recent Foraminifera, containing some new facts and hypotheses on the anatomical structure of these polypi; a further examination during the summer months of this year has enabled me to confirm the facts I have already made known, to add much new matter, and to afford such rectifications of the hypothetical inductions as will stamp them with their proper value. I persist in my view, that all the calcareous organisms styled Foraminifera are fixtures for life, as is the case with every other polyparium of the calcareous division. I considered the specimens alluded to in my first paper decidedly recent, but the possession of others which were undoubtedly alive an hour before I received them, has convinced me of my mistake. The first specimens of *Dentalina linearis* and *Marginulina legumen* exhibited in the same shell one half hyaline, and the other with the animal remains, from which I concluded that the polypi inhabited only the two or three anterior cells, and the posterior ones were rendered hyaline by the withdrawal of their contents, either by absorption or desiccation; but it is more probable that the entire shells so often met with, having all their chambers perfectly hyaline, have been cleared out, at least in those species that have decided visible apertures, by very minute parasites, and that where the chambers are partially emptied the enemy has died before its work was accomplished, in consequence of the orifice being closed up by agglutinated fine grains of sand. I confidently rely on this explanation, as in long and careful examinations of the *Miliolidæ*, I have found in them so many variously

formed parasites as to baffle, as yet, any positive determinations of the real animal inhabitant. It is necessary at once to describe the animal of *Dentalina linearis*, an inhabitant of the coralline zone of the Devon coast, six miles from shore, in fifteen fathoms water, as it appeared in a beautiful recent adult specimen of many chambers, that it may be referred to in illustration of the additional observations I propose to make. I believe they will be found more comprehensive than any that have hitherto appeared on this very distinct section of the calcareous polypi. I consider this animal and that of the *Marginulina legumen* as the types of a great majority of the Foraminifera.

Genus *Dentalina*, D'Orbigny.
Dentalina linearis, Mont.

Animal elongated, yellowish or pale red-brown; it has a continuous subcylindrical membranous tube, coasting one of the sides of the polyparium or shell from the posterior to the anterior chamber. The lobes or parenchymatous matter forming the mass of the body of the animal are deposited in the palest brown membranes, and fully fill each and every division of the shell, being moulded on their forms; these segments are united to and open into the common canal, which appears to serve for defæcation, the admission of aliment, as an oviduct, and to convey moisture to the animal: the orifice thereof is in the adult shell terminated by eight slender equidistant pale red pointed minute tentacula.

In the genial season, July and August, each lobe on its flat surface is marked with a circle of deeper red than the other part, and which I may safely term a gemmiferous pullulation, as therefrom a line of minor gemmæ is seen proceeding from each bud to the margin of the common canal to discharge therein these undoubted germs of reproduction. Thus far, as regards the animal, no doubt can exist; but with respect to respiration, the circulation, the mode of growth of the animal and polyparium, these points must be received with caution, as they have not the test of certainty, though I believe they are substantially correct. I now state what I have perceived of the increase of the animal from segment to segment, and the corresponding formation of the same parts of the polyparium. In the examination of numerous specimens of this species and of *Marginulina legumen*, in which the last chamber was incomplete and not domed over, I have seen at the neck of the antepenultimate chamber a membrane encircling and lining the unfinished wall, and a mass of parenchyma adjacent, and apparently growing *pari passu* with the common membranous tube, which is always kept free and open, and thus the lobe, tube and chambers are gradually formed

by the pullulation of the parenchyme and exudation of calcareous matter from the enveloping membrane until the lobe is complete and receives the final stigma of eight new tentacula, the old ones being merged in or become the germs of the new production, and so on, until nature has finally completed her work. We thus see that this animal, when the first germ is cast, increases by pullulation, and at the same time performs the function of reproduction by committing its gemmæ to fix themselves in their natural habitats. From these circumstances it is probable that the calcareous organisms are solitary, distributed without order, and fixed to rocks, corals, and other hard submarine substances by the pointed stylet which is attached to the posterior terminus of many of the species; and in fresh specimens of this genus and *Marginulina legumen*, the fracture of the attaching stylet is very visible by a lens of common power; but from the tenuity and fragility of the penultimate appendages, these organisms almost always come to us detached, as the substances on which they are naturally fixed are probably rocks and coral reefs; we therefore can scarcely hope to see them *in situ*; and if they ever come into the dredge on fragments, they have from their small volume been passed over without observation, and again cast into the deep. I still however hope to see them in a state of nature: I have directed my dredger to bring in all masses from the coralline zone.

To return to the animal, a curious question arises: Is it a compound being, though a solitary organism? Does the formation of gemmæ on all the lobes indicate that each is a distinct being, which, instead of opening exteriorly as in many of the other sections of the compound polypi, receives sustentation from the common canal? can this continuous tube be merely to serve as an oviduct? is it not also to supply each lobe with water, food, and for depuration? If these questions are answered in the affirmative, each segment may be so far a distinct being, as a common connection between the whole mass admits of; on the other hand, does the isochronal development of gemmæ in all, the almost isolated lobes, evidence that the animal is a simple one? If this creature had the segments inclosed in a simple tube, as in the Annelidæ, I should answer, it is not a compound animal; and perhaps even in the first case, those better qualified to judge than myself, will decide it is a simple being, and that the contemporaneous appearance of gemmæ merely shows that each lobe is under a similar stimulus.

As to the movement of the fluids, I cannot believe that the common canal serves for four distinct functions—for food, the dejections, regeneration, and aëration, without an inconvenient interference of one organ with another; I am therefore in-

clined to think there are longitudinal vessels attached to the walls of the common canal to supply some of these functions, particularly that to administer, in conjunction with the capillary filaments, the oxygen. I do not believe there is a circulation beyond that of flotation, arising from nervous contraction—I say nervous, because I shall presently enunciate the reasons for using this term. The respiration is effected by the very fine capillary filaments which issue from the foramina of such of these animals that have them, and which have been named "*pseudopodia*," or "*pedes spurii*;" the filaments are only protruded from the last-formed chambers, which, until new ones are constructed, constitute the limits of the respiratory apparatus, the preceding ones being closed by the exudation of calcareous matter from the enveloping membrane of each lobe, and though the punctures of former foramina are always seen, they are imperforate. The sustentation of these animals is undoubtedly the minute animalculæ received through the orifice into the common canal—the eight tentacula prove this—and are there digested, and the nutritive fluids enter probably by absorption into each mass of parenchyme, the rejectamenta being discharged by the aperture.

On the question of the nervous and muscular influences, which Lamarck only admits, as independent of sensation and interior sentiment, in his apathetic animals, amongst which are the Polypi, I must be allowed to make a few observations, to explain my reasons for not concurring in the views of that great naturalist. Lamarck contends that sensation, or interior sentiment, does not exist in the lower animals, and that in them all movements arise from irritabilities excited by external impressions: I demur to this doctrine, and firmly believe that no created being can exist and exhibit evidences of vitality, by motion, without having implanted in it a certain degree of sensation or interior sentiment, by the influence of which the nervous and muscular powers are put in action. I grant that external causes may produce motions and contractions, not I think by exciting an irritability independent of sensation, as Lamarck terms it, but by the agents and after the manner I have just stated.

It will be admitted that the sensations in the lower animals, which are the origin of the nervous and muscular influences, are of the most subdued qualities; and though their points of departure, and the muscular supports dependent on them, may not be discernible by the most powerful instruments, still I believe that they exist, and produce those movements which are observed in the monad as well as in man. In the superior and larger animals, we can perceive the causes of these influences and admit their existence, because they are apparent; and why not in the smallest, though they escape our vision? In the nearest fixed

stars we can observe their proper motions, but in those which are plunged in the deeper regions of the sphere, these motions, though we may presume that they undoubtedly exist, are inappreciable. Why may we not apply a similar reasoning to the doctrine of the sensations or interior sentiment, and the resulting nervous and muscular influences, being implanted in the lowest as well as the highly organized animals, according to their several structures, and not consign vast classes to exist without sensation? It appears to me that the lines of separation between apathy, sensation, interior sentiment, and intelligence, as laid down by Lamarck, are erroneous and arbitrary. I believe that apathy in its strict sense, as applied to animals, does not exist; and I repeat, that the most inferior created animal being is not without that portion of sensation or interior sentiment, and its concomitant nervous and muscular influence, that produces the motions which are the tests of vitality. I may state that Lamarck does not admit the distinction of intelligence and instinct; he very justly considers the different degrees of what is called instinct, in animals, as only subdued intelligences consequent on their imperfect organs, when compared with the highest standard —man.

There is a great gulf between the intelligence of the brute creation and that of man; the impassable line is, that the one does not fear death, and has no idea of the future, because the beneficent Creator has not given it sufficient intelligence to reason on matters which will never be granted; but man fears death and ardently desires immortality, because his Maker has conferred on him the knowledge of life and death, and it may therefore reasonably be inferred, that we shall not be tantalized with a prospective view and hope of these things, if they were not to be accomplished.

To return to the Foraminifera: I am inclined to think that the major part of these organisms, whether straight, arcuated, discoid, alternate, enveloping, rolled *en peloton*, or whatever configuration they may take, will conform in all the essential generalities with the structure of the animal—I mean of those parts of it which I have clearly determined in the *Dentalina linearis* and *Marginulina legumen*, and which I consider may fairly be constituted the type of that section of the calcareous Polypi termed Foraminifera: these organisms, from their distinct and separate growth, show an advance in organization that justly places them at the head of the calcareous Polypi, and I think it will be long before this assigned position in the progressive order of creation will be disturbed. That specialty-differences of a more or less decided character exist in the component parts of this group cannot be doubted; such variations are seen in every

division of nature. In this class the greatest deviations are the polyparia of certain of the *Nodosariæ*, improperly called *Lagenæ*, as the *L. lævis* and its variety *L. amphora*, and the *L. striata* of authors and its numerous varieties, which undoubtedly have their chambers piled on each other, and form polypiferous stems varying in the number of the strangulations of separation of one globe from another; these constrictions are often so intense, as to afford the smallest possible, often doubtful perforations; they taper from bulb to bulb, and perhaps may only be hollow on the principle of the wheat straw, to afford increased elasticity to the stems to withstand the agitation of the waters in their natural habitats of fixity. When a stem is broken into fragments, as I have seen in the *Nodosaria lævis*, the *Lagena lævis* of authors, by the mere contraction of the drying of a solution of gum arabic to fix it on a card, in consequence of the extreme brittleness of the necks of the flask-shaped globules, the terminus, or what conchologists term the aperture, will always be found under the microscope to be formed, in *fresh* specimens, of five or six rough-edged radiations, of a very different character from the symmetrical ones of those polypi that have eight tentacula, and the counterparts of these irregular radiations in shape and number will be seen at the basal part of the same object; a very strong argument that these fragments have parted from succeeding bulbs at the smallest part of the strangulation, or in other words at the aperture, leaving the base of the bulb from which it has been separated imperforate, and showing that the cylinder of strangulation is only hollow up to that point in which the principle of flexibility is involved. Conchologists have always considered the long tapering tubes, often as long or longer than the bulb itself, to be the aperture of an inclosed animal: if they are right, it must become enveloped and die, having first deposited the germ of the succeeding nodule. This unusual and extended form of the neck and aperture only exists, I believe, in two species of the entire class of Foraminifera, the *Nodosaria lævis* and *N. striata*; every other form rarely extends its neck or aperture much beyond the bulb. These two very singular exceptions, combined with the extraordinary length of the strangulations, almost amount to a demonstration, that the *Nodosaria striata*, the only organism admitting of the slightest doubt, falls into the same category as the *N. lævis*, of which I have seen a stem of four united strangulations or chambers, and others of two and three. I therefore think it not improbable that the organisms, *Nodosaria lævis* and *N. striata*, are the frames of polyparia forming stems of nodules, which, when fresh from the coral zone, are always more or less incrusted, like many of the corallines, with pulpy cretaceous matter that serves as a nidus for the mi-

nute polypiferous constructors, which may be either compound or single animals. Cabinet specimens are almost always polished by attrition.

This statement is, I believe, the true solution of the conditions of the only two Foraminifera about which doubts can exist as to the animal; all the rest, without exception, follow the type of the animals I have described above as to generalities. I may add, that I have examined with the highest powers many of the *Nodosaria striata*, and have not detected a membranous animal lining, which better observers say they have seen. When there is a minute perforation at the side of the neck of the bulb, occasioned by a boring animal, in such, the chambers sometimes contain the remains of parasites and fine mud and sand that cause discoloration of the globules, which authors may have mistaken for parenchymatous matter. It is also possible that very minute parasites may enter at the strangulated necks when the stem is broken up, and locate themselves within, in like manner as in the *Miliolidæ*, which, I have stated above, are constantly inhabited by parasites of various species. Whatever doubt may exist as to the animals of *Nodosaria lævis* and *N. striata*, I think there can be none of the *N. striata* having its unilocular globules piled one on the other. In this opinion I am strongly supported by an article in the February Number of the 'Annals' for 1849 by Mr. M'Coy, who thus observes on his *Nodosaria fusulinaformis*:—

"Shell of two or more inflated, pyriform, easily separable lodges, the first one having a small mucronate point at its posterior end, and contracted to a very slender, short neck at the anterior end which joins the pyriform second cell, which is also contracted to a similar minute neck in front; surface smooth."

Mr. M'Coy also observes, "that the lodges or cells are almost always found separated (from the minuteness of the connecting neck)." Mr. M'Coy also says, "I have however *heard* of several of them being found united in a line by their little necks, and the posterior cell not being a terminal one."

This is substantially my account of *Lagena lævis* in my first paper, and I can truly say, that Mr. M'Coy's article never came to my knowledge until long after it and the present notes were written. I have scarcely a doubt from the extracts, that these organisms are of a nearly, if not absolutely identical structure with Montagu's *Vermiculum læve*, our *Nodosaria*, and the *Lagena lævis* of authors; they have the same slender strangulations of the nodulous *Lagenæ*, the fragments of which have so long been mistaken for distinct objects. The typical *Nodosariæ* have nothing like the aspect of the very recent lageniform species, first, I

believe, introduced into that genus by myself; the necks of the typical *Nodosariæ* are strangulated, but generally so slightly as scarcely to detract from their strength, and are consequently usually found united.

It has been stated that the rough sketch of *Nodosaria lævis*, in my first paper on the Foraminifera, and which had no reference to the exact outline of that species, and was merely intended to illustrate the structure of this organism, has been mistaken by me for a *Nodosaria, not the lævis*; I therefore in decided terms state, that the mistake is not with me, and that the fragment which all authors constitute the *Vermiculum læve* of Montagu, *alias* their *Lagena lævis*, is the true and identical object I have seen in a stem of two, three, and four united nodules with elongated necks.

I exclude the family of the *Miliolidæ*, hitherto and perhaps correctly included in the Foraminifera; I have them now under investigation; and will at present only observe, that whatever their position may turn out to be, they are all inhabited by an internal animal, as my observations on the buccal pouches of the *Dentalia* sufficiently prove.

I hasten to conclude with some remarks on the neglect in which this microscopic branch of natural history has long been involved. The causes that have prevented the due consideration of the animals of the Foraminifera, and their singularly beautiful organisms, are entirely owing to mistaken ideas of the difficulties attendant on their investigation, the acquisition of the objects, and the supposed injury to the sight by the use of high microscopic powers. These objections I think I shall prove to be ideal, and if we apply the trite aphorism "Omne ignotum pro magnifico" to our case, we shall find that if we devote ourselves determinately to careful examination and investigation, all difficulties will soon disappear, and we shall be surprised at their simple solutions, because in many cases they have assumed the aspect of something miraculous, merely from being enveloped in the meshes of ignorance.

The acquisition of these elegant objects, adorned with sculpture of surpassing beauty, presents no insurmountable difficulties; every shore coated with sands has a certain line which is instantly perceived by the experienced observer, and will furnish a supply of the more common species, and the finer sands of the coralline zone, five or six miles from the shore, by the dredge, will afford abundance of the rarer species. There are also in certain districts marine deposits formed by the subsidence of the waters, which, though of great antiquity, still exhibit the freshness of recent origin without a trace of fossiliferous aspect.

As to the sight being injured by a continuous examination of

these minute objects, I can truly say that this idea is wholly without foundation if the pursuit is properly conducted, and that, on the contrary, it is materially strengthened by the use of properly adapted glasses even of high powers; and in proof I state that twenty years ago I used spectacles, but the continued and daily examination of these minutiæ has so greatly increased the power of vision, that I now read the smallest type without difficulty and without aid. The great point to be attended to is not to use a power that in the least exceeds the necessity, not to continue the exercise of vision too long, and never by artificial light, and to reserve the high powers of certain lenses and the microscope for important investigations of very moderate continuance: the really observant eye seizes at a glance the intelligence required, whilst strained, poring, and long optical exertions are delusive and unsatisfactory, and produce those fanciful imaginations of objects which have really no existence. The proper time for research after microscopic objects is for *one* hour after breakfast, when we are in the fittest state for exertion.

The very minute Foraminifera are always in fine sand, and the best way to find them is to take from the parcel of sand only as much as will lie on the point of a very small penknife blade, spreading it by a slender-pointed cedar stick on a large card, covered with dull black paper, when, with a proper lens, the objects by their symmetry and beauty are at once distinguished, and gathered up by a sable brush into proper receptacles. This apparently slow but sure mode of finding these minutiæ by purely optical exertions will produce a greater supply than by the wholesale immersion of sand in water and the resulting collection of a few buoyant objects; for after all that can be done by this mode, the sand, when abandoned, will then produce three times the number that have been acquired otherwise. In the search of shells of one-tenth inch diameter, perhaps the plan of immersion may succeed well.

Having disposed of two of the greatest drawbacks in the investigation of the Foraminifera, it only remains, as concisely as possible, to conclude the present paper by some remarks illustrative of my views in being anxious to rescue this branch of natural history from its present, I may say, retrograde position, as regards the knowledge of the animal.

The field of the British testaceous mollusca has been for many years so sedulously cultivated, that although its products are not yet exhausted, they have nevertheless become so much diminished, as is proved by the increasing far-betweens in the discovery of new species, as to render it almost a matter of necessity to look out for "fresh woods and pastures new;" and where can we find a more delightful resource, partaking so much of the

same character of our accustomed researches amongst the mollusca? Indeed the two pursuits will march in line, as the rescue from their present neglected and false position of those beautiful microscopic structures the Foraminifera, which have nearly run the gauntlet through the invertebrate portion of the order of nature in search of a resting-place. These objects are not only interesting to the mere collector, as they admit of an indefinite preservation without diminution of their singular structural and sculptural elegances, which, with lenses of ordinary powers, can be so well observed if they are properly mounted; but to the geologist the examination of these microcosms and the constructors thereof, and the bringing to light the vast numbers of still undiscovered species, are objects of the highest interest and greatest importance to assist in the solution of many intricate problems, relative to the structure, conditions, and changes of the crust of our globe.

To accomplish the important views I have endeavoured to sketch, and to infuse life, activity and interest into this portion of zoology, nothing more is required than a point of departure, which can only be effected by an energetic naturalist imbued with the "divinus afflatus," and whose years are not numbered as mine, who will undertake the useful and delightful task of giving a start, or rather an impetus to the present dormant position of this section of natural history, by throwing our indigena into divisions, genera and species, accompanied by faithful figures. As to classification, the work would be very light. We cannot adopt one characterized more concisely and distinctively than that of M. D'Orbigny, which I believe will prove more than sufficiently comprehensive for our hitherto discovered species. His first prodrome, the Foraminifera, 'Voyage dans l'Amérique méridionale, de l'île de Cuba, des îles des Canaries,' &c. &c., must form the bases of the classification. The mere substitution of one artificial system for another will be of no advantage to this branch of science, which, from its malacological neglect, must remain for some years in an unsatisfactory position, until the animals are more thoroughly investigated; and when that is done, the membranous sac, the continuous tube, the lobes from one to twenty or more, and the terminal tentacula, will form the main features of all the animals of this class, except perhaps a small section of the *Stichostegidæ*, and possibly the *Miliolidæ*; these two latter points I fully expect in the approaching summer, with the aid of the coralline zones of the South Devon coasts, to settle in such manner as will be conducive to the interest of this branch of zoology.

The principal labour would be the collection of the British articles from various cabinets; and who will hesitate to offer the

necessary contributions from his stores in furtherance of such objects, if undertaken under favourable auspices and competent qualification?

I have opened a new field for exertion, particularly for the younger naturalists, in which honour is to be acquired, and furnished in the higher walks of observation,—a new theme, and I trust that the "Hanc exorna" will be carried out with a zeal correspondent to the importance of the subject.

I am, Gentlemen, your most obedient servant,

WILLIAM CLARK.

XVII.—*On the Watery Secretion of the Leaves and Stems of the Ice-plant* (Mesembryanthemum crystallinum, *L.*). By Dr. AUGUSTUS VOELCKER, Prof. of Chemistry Royal Agricult. College, Cirencester*.

A FEW months ago I had the pleasure of communicating to the Botanical Society of Edinburgh the results of an examination of the watery liquid in the ascidia of *Nepenthes destillatoria*. Those present at the meeting, as well as the readers of the 'Annals of Natural History,' will remember that, in opposition to the statements of most botanists who have directed their attention to the subject of the watery secretions of the leaves of plants, I found the liquid in the ascidia of *Nepenthes* to differ materially from pure water, inasmuch as it contained from 0·30 to nearly 1 per cent. of solid substances, partly organic partly inorganic. I stated at that time my doubts as to the watery secretion of plants being nothing but pure water, and gave some reasons for this opinion; Prof. Balfour, with whom I discussed the subject, kindly furnished me with the means of investigating this point still further by favouring me with fresh specimens of the curious Ice-plant (*Mesembryanthemum crystallinum*), a plant which is remarkable on account of the gland-like vesicular eminences with which its leaves and stems are covered. The result of the examination of the fluid secreted by the leaves of this plant has fully confirmed the opinion expressed in regard to the watery secretions of plants; at all events it has shown me that the secretion of the leaves of the Ice-plant is not merely pure water, but water containing several substances in solution. Though I was unable to determine quantitatively the composition of this secretion on account of the small quantity of liquid at my command—a quantity insufficient even for a minute qualitative analysis—yet I had no difficulty in detecting the chief constituent parts of the fluid. The secretion I procured by lacerating the gland-like eminences with

* Read before the Botanical Society of Edinburgh, Jan. 10, 1850.